ECONOMIC METHODOLOGY AND BEHAVIORAL ECONOMICS:

AN INTERPRETIVE HISTORY

Bruce J. Caldwell

It is perhaps best to begin by stating that I am neither a psychologist nor a behavioral economist. I am an historian of economic thought whose primary area of research is economic methodology. In this paper I will present a historical survey of the development of certain methodological issues in economics over the last century. The interplay among the ideas of philosophers of science, economic methodologists, and economists proper will figure prominently in the survey. I will also attempt to link the discussion of methodology with the loosely grouped set of concepts and theories whose study is the purpose of this volume: the research program of behavioral economics.

I. THE PROFESSIONALIZATION OF ECONOMICS: 1880–1960

Those with an interest in methodology remember that an important debate, dubbed the *Methodenstreit* ("conflict of methods"), took place at the end of the last century. The two antagonists in the struggle were Carl Menger, father of the Austrian school, who argued for an abstract-theoretical approach to the study of economics, and Gustav Schmoller, leader of the German historical school and proponent of an historical-empirical methodology. But if one digs more deeply into the literature

of that time, one will discover another, less heralded debate, in which psychologists and economists crossed swords. In some future history of our discipline, it may turn out that this second debate will be judged far more significant than the *Methodenstreit*.

Between roughly 1880 and 1920, the still emergent subjective theory of value, arguably the most important result of the marginal revolution in economics, came under sustained fire from psychologists and various heterodox sympathizers in economics. The opponents of the new theory of value were armed with the psychological theories of William James, William McDougall, and John Watson. Seeking objective and measurable behavioral and physiological determinants of human behavior, they rejected the discredited hedonistic psychology that seemed to form the motivational underpinnings of the behavior of "economic man." Members of the economics profession were divided in their responses to these charges. Some were openly hostile, others were sympathetic. Significantly, most of the major figures of the day felt compelled to take some position on the issue. In the end, the controversy died down, apparently for two reasons. First, the psychologists could not settle on any single theory of motivation, so their forces became dispersed. In the meantime, economics was being transformed into a pure logic of choice. This riposte undercut the arguments from psychology, because a pure logic of choice is independent of any motivational postulates. The new science of economics was on its way toward becoming an autonomous discipline.[1]

In 1932, Lionel Robbins defended economics as a pure logic of choice in his *An Essay on the Nature and Significance of Economic Science*. Robbins confidently proclaimed that the efforts of economists had yielded "a body of generalizations whose substantial accuracy and importance are open to question only by the ignorant or the perverse" (p. 1). The truth of these generalizations cannot be established by appeals to history or by surveying the results of controlled experiments. Rather

> the propositions of economic theory, like all scientific theory, are obviously deductions from a series of postulates. And the chief of these postulates are all assumptions involving in some way simple and indisputable facts of experience. . . . These are not postulates the existence of whose counterpart in reality admits of extensive dispute once their nature is fully realized. We do not need controlled experiment to establish their validity: they are so much the stuff of our everyday experience that they have only to be stated to be recognized as obvious (pp. 78–79).

In this and other passages, Robbins argues against the claims of historicists and proponents of the experimental method. He also rejects the "behaviorist" claim that science must deal only with observable phenomena, on the grounds that explanations in economics must make reference to an individual's subjective valuation process, a process that is understandable but not observable (pp. 87–90). Finally, in defending the notion of "rational economic man," Robbins asserts that a belief in *Homo economicus* does not entail an acceptance of psychological hedonism. For Robbins, rationality implies only consistency in choice. Furthermore, he admits that under certain circumstances even such consistency may be irrational if the time and effort required for it could be better used; in his delightful prose, "the marginal utility of not bothering about marginal utility" may be a legitimate way to explain apparently inconsistent behavior (pp. 83–86, 92).

Six short years after the publication of his essay, the pure logic of choice that Lord Robbins had so eloquently defended was under attack. Psychology was not the opponent this time. Rather, it was a brash new philosophy of science, logical positivism, which had sprung up in Vienna in the 1920s and soon swept westward across Europe. A young British economist named Terence Hutchison went to Germany in the mid-1930s to study the new scientific philosophy. In 1938, Hutchison published his famous methodological treatise, *The Significance and Basic Postulates of Economic Theory*, in which the principles of logical positivism were deftly applied. The book can be read as a point-by-point empiricist assault on the ideas espoused by Robbins. As it had in so many other disciplines, positivism entered economics like a lion.

Philosophy was dominated by speculative idealism at the end of the last century, and logical positivism may be viewed as an empirical overreaction to the excesses of idealism. One goal of the logical positivist movement was to eliminate metaphysics (the bread and butter of the idealists) from the domain of science. This could be accomplished, it was believed, by the logical analysis of scientific propositions. Two sorts of statements were to be permitted in science: analytic statements and synthetic statements. Analytic statements are true or false by definition, and hence they are empirically empty. Analytic statements nonetheless had to be permitted entry into science because many scientific theories employ analytic statements in the form of identities and definitions. Synthetic statements make empirical claims about the world; their truth or falsity is contingent. One discovers whether a synthetic statement is true or false by testing it. Scientific theories would ideally contain only analytic statements and true synthetic statements. One would discover which synthetic statements were true by testing, or verifying, them. In the real world, testing is usually difficult, and the results of tests may be ambiguous. So as a practical matter, legitimate scientific theories may be distinguished from pseudoscientific metaphysics because the former, but not the latter, contain statements that are, at minimum, *conceivably testable*. Hutchison incorporates this idea early on in his essay:

> If the finished propositions of a science, as against the accessory purely logical or mathematical propositions used in many sciences, including Economics, are to have any empirical content, as the finished propositions of all sciences except of Logic and Mathematics must have, then these propositions must *conceivably* be capable of empirical testing *or be reducible to such propositions* by logical or mathematical deduction (p. 9, emphasis in the original).

This posed problems for Robbins' pure logic of choice, and especially for his defense of the rationality postulate. The first problem, soon to be solved by Samuelson's (1938, 1948, 1950) revealed preference gambit, was that agents' choices are based on subjective, unobservable, and hence untestable states of mind called preferences.[2] The second and far more lethal problem has to do with Robbins' definition of rationality, which is equivalent to consistency in choice. Reading Robbins carefully, it is evident that individuals may on occasion be *rationally inconsistent* in choice, especially when (in modern terms) information is costly. As Hutchison notes in his book, this difficulty is often overcome by defenders of the rationality postulate with the addition of another assumption, that of perfect knowledge. Since the perfect-knowledge assumption is demonstrably false empirically, such a tactic did not impress Hutchison. The major thrust of his criticism should by now be evident: Robbins' pure logic of choice fails because it is untestable. Robbins' position boils down to the assertion that agents are always rational in their choices, even when their choices appear inconsistent to outsiders. No observed behavior can falsify the rationality postulate, and as such, it is empirically empty. (Whether one then wishes to categorize it as a metaphysical or a tautological statement is another matter. For the record, Hutchison viewed it as a tautology.)

For the next twenty years, the status of the rationality postulate was hotly debated among economic methodologists. A priorists like Ludwig von Mises insisted that all human action is rational, in the sense of being purposeful, by definition. For von Mises and other adherents of praxeology, the basic postulates of the science of human action were apodictical. Hutchison, later labeled an "ultra-empiricist" by Fritz Machlup, continued to press for testable assumptions. For his part, Machlup took the middle road, arguing that theories may be "verified" by comparing their predictions, but not their assumptions, with phenomenal reality.[3]

Meanwhile, most of the rest of the economics profession was paying scant attention to the scribblings of the methodologists. The Keynesian revolution was in full bloom on both sides of the Atlantic. Econometrics was being born as the work of Schultz, Frisch, Koopmans, Tinbergen, Haavelmo, and Tintner became more widely known. At the hands of Hicks and Samuelson, dynamic analysis was developed to supplement static equilibrium theory. Von Neumann and Morgenstern gave us game theory and axiomatic utility theory; a long line of theorists built on the work of Walras, Pareto, and Cassel to construct general equilibrium theory. At less lofty heights, "applied" fields like labor and industrial organization were shedding the remnants of institutionalism as empirical tests of hypotheses became the *modus operandi* for publishable work. In a phrase, economics was becoming a *professional science*. Verbal exercises on the nature and significance of economics, the endless rehashing of seemingly unanswerable methodological questions, seemed to be useless wastes of time in the face of so many advances being made on so many fronts. Those familiar with the work of Thomas Kuhn recognize that a cyclical waxing and waning of interest in methodology is a common occurrence in science. When new paradigms emerge, and normal scientists busy themselves with "filling in the boxes" of their new theories, discussions of methodology are left behind. Kuhn's scenario is well illustrated by the economics profession in the 1950s and 1960s. All that was needed, in terms of methodology, was a simple statement of scientific procedure. As we will see, this was provided in economics by Milton Friedman (1953).

Before moving to Friedman, let us go back to the philosophy of science. Remember that Hutchison invoked logical positivism, a radically empirical philosophy of science, in his demands that economics become more scientific by becoming more testable. Logical positivism gradually fell out of vogue in the 1930s and 1940s, chiefly because it became evident that *no* science, including physics, could meet its strict standards of scientificity. By the 1950s, logical positivism had been replaced by a more moderate form of empirical philosophy, logical empiricism. Logical empiricists did not insist that every sentence in a scientific theory be either definitional or testable, since some statements in theoretical physics (e.g., those referring to atoms or forces) were neither. Instead, theories as a whole might be tested by comparing their implications, or predictions, with phenomenal reality.[4]

Though there are subtle differences of detail between Milton Friedman's brand of instrumentalism and logical empiricism, it should be evident that Friedman's views on methodology closely resemble those of the logical empiricists. According to Friedman, economic theories are tested by comparing their predictions with the data. The best predictors are the best theories, and if more than one give good predictions, choose the simplest theory. I find nothing wrong with this position if one is using a theory *solely* as an instrument for prediction. But Friedman's instrumentalism becomes a powerfully conservative force in economic methodology when one adds his further claim that the realism of assumptions does not matter. This is a step that Friedman takes on his own, and one that would not be acceptable to a logical empiricist philosopher of science.[5] In any case, the implications of Friedman's essay

for the debate we have been following are dramatic. In one sweep, any and all attempts to come up with better descriptions of the actual choice behavior of individuals, any attempts to examine the rationality postulate in economics, are declared to be unnecessary and possibly unscientific. They are unnecessary because the "realism" of assumptions does not matter. And they are unscientific because we should always choose the simplist theory, and what could be simpler than to assume that all agents maximize?

It is paradoxical that though Friedman's views on economics were scathingly criticized during the 1950s and 1960s, during the same period his methodological views were widely accepted by most of the profession. If one were to reflect on what comprises accepted scientific procedure in economics, the following set of instructions might emerge: Find a problem, model it as a maximization problem, derive some testable hypotheses (the predictions of the model), find empirical proxies for the theoretical constructs, do the econometrics, get your results. (It might well be added: If the results agree with the model, publish them; if not, find out why, and then publish them.) This description of the practices of many economists accords very well with Friedman's methodological prescriptions, and it accounts in my mind for the success of his essay.

But there is a darker side to all of this. If Friedman's views are consistent with certain ways of "doing" economics, it is inconsistent with others. In some cases it did not matter. In the 1950s and 1960s, few macroeconomists worried about using maximization models. In their pursuit of proofs of the existence, uniqueness, and stability of equilibrium, few general equilibrium theorists were bothered that their models yielded no testable implications. Yet both macroeconomics and general equilibrium theory grew in prestige.

Yet for other research programs in economics, a failure to meet Friedman's prescriptions was devastating. The traditional triad of heterodoxy—Marxian, Austrian, and institutional economics—could now be excluded from serious consideration. (These three had always been excluded, but now it could be argued that they *should* be because they were *unscientific*.) In addition to these groups, others were shut out. In particular, those who sought to analyze the actual choice behavior of agents, those who challenged the usefulness of the maximization hypothesis for understanding the behavior of individuals and firms, were given second billing. This is not to say that the work of men like Simon, Scitovsky, Cyert, March, Leibenstein, Katona, Nelson, Winter, and Williamson went unnoticed by the profession: unlike traditional heterodoxy, these people eventually got published. But for the most part, these researchers were mentioned only in passing in undergraduate texts and (with the exception of the institutions at which they taught) not at all in graduate courses.

A final point: the methodological prescriptions embraced by most of the economics profession in the 1950s and 1960s blocked the development of interdisciplinary work in the social sciences. The only type of interdisciplinary work that existed in this period was aptly labeled (and this by its defenders!) "economic imperialism." In the behavior of political agents or the foraging behavior of forest creatures or the consumption patterns of laboratory rats, the point of "economic imperialism" is not to gain insights from fields like political science or psychology or sociobiology. The point is to show that all of God's creatures, from female psychotics to congressmen to less distinguished members of the animal kingdom, maximize subject to constraints. As long as the use of constrained maximization models is viewed as the only legitimate way to model social phenomena, interdisciplinary efforts must be viewed as just another variation on the theme of "economic imperialism."

To sum up this section: Attempts to bridge some interdisciplinary gaps early in the century failed and resulted in the severing of economic theory from any considerations of psychology as economics began to be viewed as a logic of choice. When logical positivist ideas were introduced by Terence Hutchison, a number of economists interested in methodology argued about the status and content of the rationality, or maximization, postulate. Meanwhile, economics was becoming more professional, as theories were expressed in mathematical form and econometric techniques became increasingly sophisticated. The methodological debates of old seemed dated and otiose in the face of such advances. Friedman's methodology of positive economics, on the other hand, fit the times nicely. Not all of established economics fit its mold, but enough of it did. And it instructed economists to exclude from consideration, on the grounds that they were *unscientific*, a number of alternative approaches to understanding economic reality. Thus we find that methodology, a term so often associated with the interplay of diverse approaches to a subject, became a powerfully conservative force in the 1950s and 1960s. It is no small irony that many mainstream economists of this period thought that the study of methodology was a waste of time, while at the same time they believed that alternative approaches to the study of economics should be excluded *on methodological grounds*!

II. PHILOSOPHY AND ECONOMIC METHODOLOGY IN THE POSTPOSITIVIST PERIOD

During the same time period that economic methodology was entering its conservative stage, a revolution of enormous consequence was taking place in the philosophy of science. The *ancien régime* was logical empiricism, the latest and most mature variant of positivism. The revolutionaries were neither radicals nor reactionaries. They were historians and philosophers of natural science, and some of their names have become familiar, even to economists: Karl Popper, Thomas Kuhn, Imre Lakatos, Norwood Hanson, Stephen Toulmin, Paul Feyerabend. In the course of twenty years, the revolution succeeded. Positivism was overthrown; the postpositivist era had dawned.

Little of the logical empiricist structure withstood the attack. A number of types of scientific explanation (motivational, functional, genetic, historical), which were considered legitimate explanations in their respective fields, could not be squared with the famous "covering-law" models of explanation. The hypothetico-deductive model of theory structure depended for its force on a one-to-one correspondence between, on the one hand, theoretical terms and nonobservable entities, and on the other, nontheoretical terms and observable entities. Too often the one-to-one correspondence did not exist. The "symmetry thesis," which states that all legitimate scientific explanations must be transformable into predictions, was frequently violated. Evolutionary theory cannot predict but can explain which species are selected to survive; one can explain a suicide without being able to predict it: must such explanations be deemed unscientific? Confirmationism stated that the most highly confirmed theories were the best theories. But certain "paradoxes of confirmation" showed that confirmations are easy to multiply (a non-black non-raven, e.g., a white shoe, confirms the statement, "All ravens are black"), and Popper's critique of inductivism challenged the idea that high confirmation is even desirable. Simply put, if the tenets of logical empiricism were ever taken seriously (they never were followed by scientists), most of what we call science would be deemed unscientific.[6]

More important than the details of the case against logical empiricism are the new visions of the scientific enterprise that have emerged in the last twenty-five years. I use the plural of *vision* because no unified, monolithic approach has yet arisen in response to the failures of positivism. But certain common themes are in evidence. Logical empiricists attempted to articulate universal models and procedural rules which they felt were characteristic of legitimate scientific practice. Postpositivists instead emphasize the growth of knowledge over time, the dynamics of change within individual disciplines, and the actual practice of scientists. Logical empiricists used the tools of logic in their analyses. Postpositivists attempt to interpret the development of specific research programs in specific disciplines, and often make use of tools from such disciplines as history, sociology, linguistics, and even psychology in their rational reconstructions. Logical empiricists sought a definition of scientific rationality that was absolute, universal, and immutable. Having recognized with Thomas Kuhn that "paradigm-switching" is seldom a rational affair, or with Lakatos that "instant rationality" is impossible when two research programs collide, post-positivists seek a definition of scientific rationality that is organic, contingent, and flexible. It should be evident that there are dangers of relativistic and skeptical excesses in the new environment. Most of the philosophers mentioned earlier eschew such excesses. But if the dangers are great, so are the rewards: namely, a more complete and honest understanding of the nature of science, in all its variety and complexity.

It may finally be mentioned that the revolution in philosophy is by now spreading to the special sciences. Within economics, there has been a massive increase in interest in the study of methodology. Like philosophers before them, economic methodologists are asking many more and different types of questions and are borrowing from fields like rhetoric, sociology, history, and philosophy in their attempts to formulate answers. Books on methodology have multiplied, sessions on methodological topics are being scheduled at academic conferences, articles on a wide variety of topics are appearing in journals.[7] This is not to say that a heightened methodological consciousness is sweeping the discipline. But it is a welcome change from twenty or even ten years ago, when the only topic considered worthy of discussion was Friedman's essay.

III. BEHAVIORAL ECONOMICS IN THE POSTPOSITIVIST PERIOD

What are the implications of all this for the behavioral economics research program?

First of all, at least one objection to the proliferation of alternative research programs, that provided by strict positivist restrictions on what constitutes legitimate scientific behavior, has been substantially overcome. The implication in economics is clear. One may no longer argue that the use of a maximization model that yields testable implications is the *only* permissible way to do economic science. If, instead of using the rationality assumption, an economist chooses to begin his analysis with some other assumption about an agent's decision-making process (be that satisficing, rule-of-thumb behavior, imitative behavior, routine-following, or something else), such approaches may no longer be judged a priori as being less scientific. If an economist presents a model whose virtues are to explain or describe rather than predict behavior, his opponents who insist that a model *must* have predictive content may no longer look to the philosophy of science to buttress their claims.[8]

An example will show that this claim is not as provocative as it might

at first appear. In a recent article, Paul Schoemaker (1982) notes that a theory may be assessed according to its objectives. He shows that expected utility models have been used descriptively, predictively, postdictively, and normatively, and then assesses the theory according to how well it performs these various functions (pp. 538-541). For example, in examining how well the expected utility model describes the actual choice behavior of agents, he reviews a number of studies in which the axioms of the model are frequently violated (pp. 541-548). He also notes alternatives to the model, for example, psychologists Kahneman and Tversky's prospect theory (1979), that help account for some of the anomalies. Such work would be judged scientific by most observers. A positivist economist, however, would insist that tests of the axioms and alternative explanations of violations are unnecessary and unscientific. The axioms are "unrealistic" assumptions; they should not be directly tested; their worth may only be assessed by how well a theory employing the assumptions predicts. (As Schoemaker shows, the expected utility model does not work well in terms of prediction either, but that is another story.)

Another insight attributable to the new philosophy of science is the recognition of the importance of tradition in science. The scientific enterprise is a conservative one; change does not occur quickly. Kuhn points out that, even if a paradigm is shot through with anomalies, a scientific revolution will not occur until an alternative paradigm emerges. Lakatos (1970) notes that a research program may degenerate for a long time before it is superseded. Such conservative tendencies are beneficial in certain respects: When the best minds of a scientific community are all focused on working within a single paradigm, great advances are possible. But there are costs as well, and these are borne by those who wish to change the direction of scientific research. Questions of strategy arise, then, if proponents of behavioral economics want their approach to be viewed as a scientific revolution. If we take to heart Kuhn's observation that theories are never successfully challenged by anomalous facts, but only by other theories, then it is clear that a synthesis of ideas must occur among behavioral economists. As I read some of the contributions of behavioral economists—for example, Leibenstein's "Micro-micro Theory" paper, or Nelson and Winter's book, *An Evolutionary Theory of Economic Change* (1982), I noticed the tendency of these authors to distinguish their own work not only from neoclassical economics but also from other critics of the maximization model. Such distinctions are valuable and necessary for clarifying the nuances of various approaches, but they are best left in the background if it is one's intention to construct an alternative research program. Behavioral economics, then, may need an Alfred Marshall—someone who can bring together a number of disparate strands of thought into a unified whole.

Given the large number of pretenders to the throne of neoclassical economics (a throne, it should be mentioned, that has yet to be vacated), the more modest strategy of aiming at a synthesis between behavioral and neoclassical economics has a certain appeal. Nelson and Winter (1982) sometimes seem to take this approach in their superlative book. Borrowing analogies from evolutionary theory and biology, they come up with theories of decision-making in the firm and of firm survivorship that are much richer than the neoclassical view. Yet they also constrain their models to yield many of the same predictions that emerge from standard analysis. At least for this reviewer, such an approach is eminently sensible: the baby has been distinguished from the bath water.

A final strategic point is to get some of these ideas into the textbooks. In an article in which he disparaged the survival of the kinked oligopoly demand curve in intermediate microeconomics texts, George Stigler wrote, "The textbooks of a discipline play a powerfully conservative role in the transmission of doctrine" (Stigler, 1978, p. 200). Stigler's point is that it is nearly impossible, once an idea gets into the textbooks of a discipline, to get it out. The converse is also true: new ideas are not easily added to textbooks. In addition to a Marshall, then, behavioral economics may need a Samuelson. I am not talking about the Samuelson of the *Foundations*, but rather the Samuelson of the *Principles*: the Samuelson who introduced the Keynesian cross to generations of introductory economics students here and around the world. If behavioral economics can find a Marshall and a Samuelson—that is to say, a synthesizer and a popularizer—the prospects for a successful interdisciplinary revolution will be improved. But again, that none of this will be easy should be understood at the outset.

Putting questions of strategy aside, I will end these musings with what I see as the ray of hope in all this. It is a ray of hope for progress in the long run and, as such, involves what is best described as a wishful prophecy. Many of the papers in this volume were presented at a conference attended primarily by psychologists and economists, though a few other disciplines were also represented. At one point in the discussion, it was noted that behavioral economics should not be considered the exclusive province of psychology and economics, and everyone present agreed. More to the point, the sort of interest in genuine intercourse across disciplines exhibited at the conference was, in my experience, both unique and refreshing. This is not to say that communication was always easy. As an economist, I found that I could usually understand the points made by economists, but that I was often confused about why the psychologists asked the questions they asked and why they chose to answer them in such curious ways. Such confusion may cause some to despair, since it is only the first obstacle in a movement toward mutual understanding.

But putting the best face on it, perhaps these and other obstacles may be overcome, so that the social science of the next century will be a truly interdisciplinary endeavor. We saw earlier in this paper that a movement toward autonomy by economics in the first part of the twentieth century was probably a necessary first step for it to become a professional discipline. That autonomy allowed economics to advance as a science. Perhaps the next stage in its advancement will be a movement toward becoming a part of a larger science of society. If such a step occurs, the place of behavioral economics in the advancement of our knowledge of society will loom larger than even its fondest proponents might now imagine.

NOTES

1. For a more detailed examination of this debate, see Coats (1976).
2. Whether Samuelson's revealed preference program actually succeeded in accomplishing its goals is contested by Wong (1978).
3. See von Mises (1949); Machlup (1955, 1956); Hutchison (1956).
4. For critical surveys of these and other issues in twentieth-century philosophy of science, see Suppe (1977), Blaug (1980), and Caldwell (1982).
5. This is why in a recent discussion, Friedman was characterized as an instrumentalist rather than as a positivist or a logical empiricist. See, for example, Boland (1979), Caldwell (1980).
6. Again, these developments are surveyed in the citations in note 4.
7. The size of the bibliography in Caldwell (1984) supports this claim.
8. Of course, this does not mean that a novel approach is necessarily better. All I am saying is that new approaches should not be judged inadequate *solely* because they are new.

REFERENCES

Blaug, Mark, *The Methodology of Economics: Or How Economists Explain*. Cambridge: Cambridge University Press, 1980.

Boland, Lawrence, "A Critique of Friedman's Critics," *Journal of Economic Literature*, 17 (June 1979), 503–522.

Caldwell, Bruce, "A Critique of Friedman's Methodological Instrumentalism," *Southern Economic Journal*, 47 (October 1980), 366–374.

Caldwell, Bruce, *Beyond Positivism: Economic Methodology in the Twentieth Century*. London: Allen and Unwin, 1982.

Caldwell, Bruce, *Appraisal and Criticism in Economics: A Book of Readings*. London: Allen and Unwin, 1984.

Coats, A.W., "Economics and Psychology: The Death and Resurrection of a Research Program," in Spiro Latsis, ed., *Method and Appraisal in Economics*. Cambridge: Cambridge University Press, 1976, pp. 43–64.

Friedman, Milton, "The Methodology of Positive Economics," in *Essays in Positive Economics*. Chicago: University of Chicago Press, 1953, pp. 3–43.

Hutchison, T.W., *The Significance and Basic Postulates of Economic Theory*, 1938; reprint ed. New York: Augustus Kelley, 1960.

Hutchison, T.W., "Professor Machlup on Verification in Economics," *Southern Economic Journal*, 22 (April 1956), 476–483.

Kahneman, Daniel and Amos Tversky, "Prospect Theory: An Analysis of Decision Under Risk," *Econometrica*, 47 (March 1979), 263–291.

Lakatos, Imre, "Falsification and the Methodology of Scientific Research Programmes," in I. Lakatos and A. Musgrave, eds. *Criticism and the Growth of Knowledge*. Cambridge: Cambridge University Press, 1970.

Machlup, Fritz, "The Problem of Verification in Economics," *Southern Economic Journal*, 22 (July 1955), 483–493.

von Mises, Ludwig, *Human Action: A Treatise on Economics*. New Haven, CT: Yale University Press, 1949.

Nelson, Richard and Sidney Winter, *An Evolutionary Theory of Economic Change*. Cambridge, MA: Harvard University Press, 1982.

Robbins, Lionel, *An Essay on the Nature and Significance of Economic Science*, 1932, 2nd ed. London: Macmillan, 1935.

Samuelson, Paul, "A Note of the Pure Theory of Consumer's Behavior," *Economica*, 5 (February 1938), 51–61.

Samuelson, Paul, "Consumption Theory in Terms of Revealed Preference," *Economica*, 15 (November 1948), 243–253.

Samuelson, Paul, "The Problem of Integrability in Utility Theory," *Economica*, 17 (November 1950), pp. 355–385.

Schoemaker, Paul, "The Expected Utility Model: Its Variants, Purposes, Evidence and Limitations," *Journal of Economic Literature*, 20 (June 1982), 529–563.

Stigler, George, "The Literature of Economics: The Case of the Kinked Oligopoly Demand Curve," *Economic Inquiry*, 16 (April 1978), 185–204.

Suppe, Frederick, ed., *The Structure of Scientific Theories*, 1973; 2nd ed. Urbana, IL: University of Illinois Press, 1977.

Wong, Stanley, *The Foundations of Paul Samuelson's Revealed Preference Theory: A Study by the Method of Rational Reconstruction*. London: Routledge and Kegan Paul, 1978.